tween the ice and the meteorological conditions based upon the last 10 years of the ice patrol work. Difficulty has been experienced in securing meteorological records from critical points on the Greenland and North American coasts.

It is unfortunate that there are not several year-round meteorological stations in northern regions. Besides the advantage which might be derived from their records, as just indicated, they might also serve as ice observation posts. If a station could be located somewhere along the side of the arctic drift where it sweeps in close to the shore, for instance at Cape Dyer, Baffin Land, it could serve the double purpose of a meteorological station and an ice observation post. The situation may be likened to that of a river. Flotsam observed upstream in the current will later appear at the river mouth. In this case the Labrador Current is the river whose mouth is in the vicinity of the Great Bank of Newfoundland; the flotsam is the icebergs. It takes approximately five months for a berg passing Cape Dyer to appear south of the 45th parallel. If the record of the number of bergs, with dates of passing Cape Dyer, were known to the ice patrol and the Hydrographic Office, long range forecasting of ice conditions in the North Atlantic would probably be possible. It would prepare us to meet and deal with a situation about which to-day we lack advance information.

POLAR ICE-DRIFT AND SUN SPOTS.

By GEORGE NICOLAS IFFT, American Consul.

[Bergen, Norway, Dec. 6, 1922.]

An interview with Dr. Adolf Hoel expressing doubt of the possibility of Amundsen's plan for drifting over the North Pole in the *Maud* with the supposed drift of the polar ice is attracting much attention throughout Norway and causing considerable discussion in the Norwegian press. Doctor Hoel, who is lecturer on geology at the Christiania University and who during the summer headed a government research expedition to Spitzbergen and the surrounding waters (see my report on "The Changing Arctic," transmitted under date of October 10, 1922), suggests that such drift over the pole would be possible, if at all possible, some years hence, upon the theory that the polar region is subject to fixed periodic changes and that such period affecting ice conditions is one of from 10 to 11 years closely connected with the known sun-spot periods.

Doctor Hoel states that the fact of the ice drift from the northern coasts of Asia and America across the pole to the strait between Spitzbergen and Greenland and then south along the east coast of Greenland has been shown by the drift of the *Jeanette* and other vessels. Amundsen's experience last year, however, seemed to indicate that the ice drift is subject to variations. At all events, the *Maud* did not succeed in getting into the drift because of unfavorable ice conditions and Doctor Hoel argues that it is reasonable to assume, either, that the exceptionally favorable ice conditions now prevailing at Spitzbergen are due to the fact that the polar current is weak and that the unfavorable ice conditions on the Asian and American north coasts are due to such cause or, that the ice in those regions actually moves in an opposite direction from that in which it has been believed to move.

Dr. H. T. Hesselberg, director of the Norwegian Meteorological Institute, discussing such suggestion, states that there can hardly be talk of a 10 or 11 year ice period in the polar seas without having submitted such theory to a thorough investigation and without a thorough study of the comparatively scanty material at hand. In regard to a relation between polar ice conditions and sun-spot periods, he said that the influence of sun spots is felt in so many conditions, among them atmospheric conditions, that it is not impossible that they also play their part in ice conditions about the pole. At the same time, he considers Doctor Hoel's statement of the utmost interest, as he is thoroughly familiar with conditions in that section of the world.

A REVIEW OF GEOPHYSICAL MEMOIRS NO. 19.[1]

By ALFRED J. HENRY.

[Weather Bureau, Washington, D. C., Dec. 28, 1922.]

The latest *Memoir* of the British Meteorological Office is a welcome contribution upon a subject of very great interest from both a theoretical and a practical viewpoint. It is peculiarly appropriate that this discussion of tropical cyclones should come from the English Meteorological Office, since it was Piddington, an Englishman, who first gave the name cyclone to the revolving storms of the Bay of Bengal more than half a century ago.

The raison d'être of the *Memoir* was an inquiry originating with the Colonial Secretary as to the visitation by tropical storms to the various dominions beyond the seas. Naturally the Meteorological Office was called upon to prosecute the inquiry. Obviously one of the first steps was to assemble in convenient form the enormous mass of widely scattered material from the original sources. The accomplishment of this object was entrusted to Mrs. E. V. Newnham, M. Sc., a member of the professional staff of the forecast division. How well she accomplished this difficult task may be seen by a perusal of the 102 closely packed quarto pages of text and charts.

The *Memoir* includes, in addition to the material collected by Mrs. Newnham, an introduction by Sir Napier Shaw, to which reference will be made later, and a short discussion by Dr. Harold Jeffreys on "Theories on the Origin of Tropical Cyclones."

The observational material is presented in four sections, each one dealing with those portions of the great oceans which are subject to visitation by tropical cyclones. These are:

(1) North Atlantic: A. West Indian Hurricanes.
B. Squalls and Tornadoes of West Africa.

(2) Indian Ocean: A. Cyclones of the Bay of Bengal and the Arabian Sea.
B. Cyclones of the South Indian Ocean.

(3) Pacific Ocean: A. Typhoons of the North Pacific.
B. Revolving Storms of the South Pacific.

The material is presented in great detail with many rather full extracts from the original papers. Thirty-three full page plates with numerous inserts illustrate the paper.

[1] Hurricanes and Tropical Revolving Storms, by Mrs. E. V. Newnham, M. Sc. With an introduction on The Birth and Death of Cyclones. By Sir Napier Shaw, F. R. S. pp. vi. 122 illus. H. M. S. O., 1922. Price 12s. 6d.

The indtrouction by Sir Napier Shaw was delivered as a lecture at a meteorological conference at Bergen, in July, 1920. The lecture begins with a short discussion of the maintenance and structure of cyclonic depressions from which the lecturer passes to a consideration of the subject of a revolving fluid in the atmosphere, a matter upon which he had made previous studies. It is specifically pointed out that if the fluid were carried along by a current the winds would represent not simply the rotation but combination of translation with rotation, a fact that is sometimes overlooked.

The subject of the lecture is further discussed under the following heads:

- Examples of cyclonic circulation.
- Localities of cyclonic depressions and tropical revolving storms in relation to the polar front.
- The extension of the polar front to the equatorial zone.
- The tropical anticyclones.
- The places of origin of tropical revolving storms.
- The thermal convection of hot moist air.
- The birth of a tropical cyclone.
- Precipitation.
- The death of cyclones.
- The height of cyclones.
- Descending and ascending air.

Each of the above topics is full of interest, especially to those who have been seeking to reconcile existing theories on the origin of cyclones. Since space will not permit a full abstract, the topic of greatest interest to REVIEW readers—"The birth of a tropical cyclone" has been selected for presentation in the fullness of the original.

In the immediately preceding section upon the thermal convection of hot, moist air, reference is made to certain sounding-balloon satistics for Java which give the normal lapse rate of temperature with height in the equatorial region. From Neuhoff's diagram [3] and equation the effect upon temperature of adiabatic changes of pressure in the case of air saturated with water vapor at, for example, 300° A. (about 80° F.) can be determined. Setting these data side by side it is seen at once that air saturated with water vapor at 300° A. would be in unstable equilibrium at Batavia, Java. If it began to rise it would not find itself at the same temperature with its surroundings, and therefore not permanently in equilibrium, until the level of 15 km. was reached, and only then if we suppose it to be loaded with its condensed water as drops. After they had fallen out, further height would be required to bring the density of the rising air to that of its environment.

Furthermore we may consider what would be the pressure at the surface if a column of air some 10 or 20 miles in diameter, for example, were replaced by the air which was saturated at the surface and thrust up into the heights. The pressure difference between an air column so defined and its environment can be computed, neglecting the humidity of the air in computing the density but allowing for it in the temperature. It appears that in these circumstances the difference in pressure between the exterior column and the environment would be as much as 81 mb. at the surface, gradually decreasing from that amount to 8 mb. at the level of 10 km. and to nothing at the level of 15 km. These facts are presented in the following table:

[3] Smithsonian Miscellaneous Collections, vol. 51, no. 4, 1910.

TABLE 1.—*Normal pressures and temperatures in equatorial air (Batavia) with the temperatures of air saturated with water vapor at 300° A. and reduced without any supply of heat to the pressure at the uppermost level, with the differences of pressure at different levels between the normal air and the column of saturated air.*

Height.	Normal air Batavia.		Saturated air changed adiabatically to same pressure at 15 k.		Pressure difference between the two columns.
	Pressure.	Temperature.	Temperature.	Pressure.	
k.	*mb.*	*°A.*	*°A.*	*mb.*	*mb.*
15	128	198	199	128	0
14	152	203	209	151	1
12	209	219	229	207	2
10	283	235	248	275	8
8	376	251	263	360	16
6	491	265	275	464	27
4	632	279	284	591	41
2	803	290	292	745	58
1	903	295	296	835	68
0	1,012	300	300	931	81

From the facts of the above table it is pointed out that this form of instability is very much dependent upon the temperature of saturation of the air, and it is therefore limited to regions where the air is not only very hot, but also very moist. Also, that the difference of temperature between the rising air and its environment reaches a maximum of 13° A. at 10 km.

It further appears that under suitable conditions the air of the surface is capable of rising to the heights which are actually characterized by convection in the equatorial regions, and that if a hollow column could be filled with it and protected by a rigid wall from its environment it would give rise to a difference of pressure at the surface of the same order as those found between the centers and margins of tropical cyclones and rather larger than is generally observed. The question arises as to how a hollow column can be provided automatically which will fill itself with air of suitable composition and temperature without bulging or collapsing. The author assumes, in explanation of this difficulty, that the interior column is protected dynamically by the spin of the surrounding air, and that the necessary velocity of rotation has been acquired by carrying away the air which originally filled the space now occupied by the interior column (and much more besides) in order that the convergence of the environment toward the region from which the air has been removed may develop the angular velocity of rotation necessary to provide a stable system with a core of very low pressure.

The foregoing serves as an introduction to the formal outline of the conditions which are concerned in the origin of a tropical cyclone. The ideas of Sir Napier Shaw on this very interesting and complex problem are given in his own words in the next section.

THE BIRTH OF A TROPICAL CYCLONE.

If we agree that the situation which is thus disclosed is strongly in favor of convection from the surface as the real agency in the formation of a tropical cyclone, we have still to consider the manner in which convection could produce the result. We have to recognize that *the first stage* is the removal of a very large volume of air *at all heights* so that the air at all levels may converge toward the axis and cause the superposition of a simple vortex upon the original rotational condition of the air.

It may here be remarked that, if there is no general tendency toward rotation which can be developed by convergence, the instability of the air will only be attended by a local shower and local disturbances of wind, such perhaps as those of the doldrums, which are too near the Equator for the earth's rotation to be effective in originating a vortex.

The traditional view of the process of convection in meteorology may be described as the formation of a continuous local circulation, with a vertical portion caused by the continuous ascent of air in a particular locality in consequence of its relatively high temperature and a horizontal portion for the continuous replacement of the rising air by the pressing forward of colder air, which approaches from a considerable distance and itself becomes warm enough for ascent by the time it comes to the proper locality. The process is pictured in imitation of the continuous circulation which is set up in a vessel of water when one part of the bottom of the vessel is heated or in a system of heating by hot-water pipes; but in the atmosphere conditions are different; dynamical cooling introduces modifications which make the establishment of a continuous circulation, on the model of the laboratory or of the hot-water engineer, very difficult to trace or to imagine. The ascent of air becomes a question not merely of local warming, but of environment as well.

And apart from this fundamental difficulty the continuous pushing upward of a supply of air by distant pressure, acting like a continuous piston moving inward, would not provide for the necessary abstraction of air at all levels. On the contrary, it would seem to suggest the bulging of the sides of the column outward by the intrusive air. The traditional explanation does not take proper account of the fact that air only goes upward when it is pushed up.

Let us therefore consider more closely the process of convection. In the atmosphere convection may apparently proceed either by threads or bubbles. By the thread process, which may be operative on a sunny day when the surface is solarized, a thick layer covering an enormous area may be gradually brought into the condition of convective equilibrium for dry air. This is probably the case with the air over the Sahara or any other hot desert. The process is different from the formation of a huge bubble separated from the mass below through undercutting by the inflow of cooler air in the neighborhood. What conditions are necessary for the formation of bubbles on a very huge scale are not known, but it seems certain that when condensation begins bubble formation must be set up.

When a large bubble forms, it is pushed upward by the convergence of the air beneath it, and it pushes aside the air above it, the final result of the ascent of a single bubble being the convergence of the surrounding air at the level where the bubble was first formed. But as it passes upward eddies will be formed on its exterior, and some of the original column will be dragged up with it at the expense of some ascensional force. If we conceive the process of convection as the passage upward of a succession of innumerable large bubbles somewhat in the same manner as the escape of air from the neck of a bottle completely submerged, until many cubic miles of air have been lifted, the air originally over the area will have been gradually removed; the external air will have converged toward an axis and the beginnings of eevolving fluid will have been set up by the dynamical consequences of the original thermal process. Continued further, the same process will continue to remove the internal portion of the revolving column until the rotation has become sufficiently developed to resist further convergence toward the center. By that time, with the aid of the original vorticity of the earth's rotation, we shall have reached at all levels the condition of a simple vortex with a ring of maximum within which the pressure is kept low through the continual removal of air by what may be called the scouring action of the ascending bubbles. The axis then becomes practically unapproachable because the air that aims toward it is always deviated from its course. It takes part in the circulation and misses the convergence. So we get a dynamical system of great stability which admits air to the region of the axis only along the immediate surface, where the motion can not each the limit of protection, because it is retarded by friction.

So far we have a warm core with an environment the temperature of which, except at the very bottom, is governed by the dynamical cooling due to the convergence toward the axis. If the air of the environment contains sufficient moisture, cloud will form; and with the formation of cloud instability is probable, which will cause further condensation and possibly abundant rainfall outside the original column. All this can occur while the whole system is being developed in the easterly wind, and it moves with the wind toward the region where the surface water is still warmer, and consequently the surface air also becomes warmer; the dynamical process of scouring the central column is continued. But there will come a time when the supply of hot moist air at the surface is exhausted, and then the passage of the air through the column by ascent from the bottom must cease; the air can only rise until its temperature is the same as that of the cooled environment. When that stage has been reached, any hot air remaining in the column will be ejected at the top by the convergence from the sides, and we shall have obtained a dynamical system consisting of a vortex with a ring of maximum velocity of finite diameter and its interior protected from further invasion, except at the bottom, by the velocity of rotation, so that it can only be affected by the creeping of air or other material into the interior along the bottom. The temperature distribution will be that produced by convergence of the environment toward the axis; the whole effect of the convection, originally due to the heated and saturated surface air, will have been to cause the removal of the air from along the axis, which Lord Rayleigh's exposition requires for the formation of a vortex of revolving fluid. Thus the high temperature of the interior is merely a temporary incident in the formation of a cyclone vortex; by the time the vortex is developed as a dynamical system the core is cold; there is no longer any convection in it; it becomes a comparatively small area, protected from the ordinary vicissitudes of weather by the enormous momentum of a vortex with a high rate of spin, represented by the very violent winds of a certain ring, but extending in less violent form over a vast area.

It may be noticed that the ultimate violence of the winds of the maximum ring depends on the limitation of the area of convection. Since the velocity in the vortex varies inversely as the distance from the axis, if convection can be effective in removing the necessary amount of air without using an area greater than a half kilometer in diameter, the wind at a distance of 1 kilometer from the axis will be only one-quarter of the maximum. The shape of the curve $v_2r=$ constant, in Figure 1 [not reproduced] will approach much more nearly to the two axes.

The discussion on "Theories of the Origin of Tropical Cyclones," by Dr. Harold Jeffreys in the following paragraphs is most helpful to a clear understanding of this complex problem:

THEORIES ON THE ORIGIN OF TROPICAL CYCLONES.

The cause and dynamics of tropical storms are still very imperfectly understood, but a few important features are fairly clear. It is a well-known phenomenon that when a body is in slow rotation at the commencement, and is drawn toward some fixed center the velocity of each particle of the fluid increases as it approaches the center. If the motion is perfectly symmetrical, the velocity of a particular particle about the center is inversely proportional to the radial distance. The most familiar instance of this is the swirling motion that is developed in a bath when the plug is withdrawn from the bottom; the effect of the displacement of the water, from an average distance of a few feet from the center, to about an inch from it, is sufficient to increase the inappreciable rotation that was present at the commencement into a vigorous vortex. There can be little doubt that the rotation of a cyclone is produced by such a displacement toward the center. The initial rotation in this case arises chiefly from the rotation of the earth, since the actual motion of the air at any place is the resultant of the motion of the ground below it and the observable wind, the former of which is by far the greater. If now the center be north of the Equator, we see that the true eastward velocity north of it due to rotation of the earth is less than the velocity on the south side, the difference being such as would be produced by a counterclockwise rotation about the vertical. This being magnified by an approach of the air to the center, a counterclockwise whirl is developed. The opposite is true in the southern hemisphere. Thus, if this analogy is correct, all cyclones in the Northern Hemisphere should rotate counterclockwise, while all those in the Southern Hemisphere should rotate clockwise. This invariable difference in the directions of rotation of storms north and south of the Equator is one of the best known facts about these storms, and affords an immediate and convincing verification of this part of the theory.

If, however, the surrounding air near the ground approached the center without any outward displacement taking place above, there would be an accumulation of air above the central region, and consequently a real increase of atmospheric pressure there; whereas we know that the pressure in the middle of a cyclone is lower than elsewhere. Hence, while the air on the ground is moving inward, that above it must move outward; and the reduction of pressure inside shows that the amount of air that moves outward above must be greater than the amount that moves inward below. Such an outward motion of the upper clouds is suggested, though scarcely proved, by the distribution of the "cirrus," which is usually described as radiating from the center.

As the outward and inward motions appear, so far as can be detected from the phenomena observed while the cyclone is developing, to occur simultaneously, it is not easy to say which of them is cause and which is effect. It is natural, however, to suggest that the outward motion, being the greater, is the cause of the other. Now if such an outward displacement took place in the upper air, it would leave behind it a region of reduced pressure, and the lower air would flow in toward the center on account of this. Further, it is unlikely, on account of inertia and friction, that this would take place so rapidly as to neutralize the diminution of pressure completely, so that a low pressure would remain. Thus an outward displacement of the upper air affords an adequate explanation of the distribution of wind and pressure at the surface.

Two types of theory have been advanced to account for tropical cyclones; in their essential features they are both consistent with what has already been said, but they differ in the causes assigned to the outward displacement, which is a necessary feature of both. The first of these is the convectional theory. This requires a local heated region, over which a column of very moist and warm air develops. The initial effect of both the heating and the evaporation is to cause an increase in the volume of the air affected, and hence the upper air is lifted up by the expansion below it. It then flows out so as to readjust its level, giving the outward displacement we need for our theory. The formation of clouds and rain in the lower air is a consequence of the fact that as the air moves inward it comes to a region of lower pressure, where it expands and cools to some extent, and consequently can not retain all the water vapor it held previously.

One difficulty of this theory is that these storms always form over the ocean, and always in summer. The land in summer becomes much hotter than the sea, and therefore we might expect on this theory that more cyclones would take place on land than at sea, whereas actually they all originate at sea. This objection would be met if it were shown that moisture is much more important in producing these distrubances than a rise in the temperature of the ground, equal to the difference between the summer temperatures of land and sea, would be. For this to be true requires a very high vapor pressure in the saturated air, which can only be obtained near the equator. This may, therefore, be the reason why revolving storms of this type are confined to the tropics.

Another difficulty is that the conditions over vast areas of the ocean must be very uniform, and that there is little reason why one region 200 kilometers across should be singled out as the place of origin of a storm rather than any other. It may be, however, that the whole of an extensive region is on the verge of developing into cyclones, and that only a trifling difference is needed to localize the disturbance when it develops. A way in which an outward displacement may arise from such a difference and lead to a persistent storm is described by Sir Napier Shaw in his introduction.

It may be remarked that it is not necessary to the continuance of a storm that the causes that brought it into being should retain all their efficacy. In particular, it is not necessary that a strong vertical current should persist. When the revolving column is started, mere inertia will keep it going for a considerable time in spite of friction. This is probably the chief reason why such storms are able to move so far toward the poles when they have once been formed.

A revolving storm would naturally be expected to have a motion of translation on this theory. The conditions of its formation require it to start in the Tropics, but not at the equator, since a storm formed at the Equator could acquire no rotation. Thus all such storms start in the regions of the trade winds, and the air forming their cores has initially the general motion of the places where they form. It retains this, only changing it in consequence of the widespread pressure difference in the regions through which it passes. Thus these storms have usually velocities of translation not very different from those of the general winds of their surroundings.

The chief alternative theory is the mechanical theory first suggested by Dove, supported by Thom, Meldrum, and Fassig. The formation of circular whirls at the boundary between opposing currents of water in a millpond is wellknown, and this theory suggests that tropical cyclones develop in much the same manner at the boundary between two winds of extent comparable with 1,000 kilometers or more. The velocities in these millpond eddies, however, never exceed those in the main current, while those in tropical cyclones always do so. In the absence of quantitative dynamical investigation, it would therefore be very dangerous to adopt the theory in this simple form as an explanation of tropical storms, though it might do for those of the Temperate Zone. Nevertheless it is true that such conflicting winds do exist on opposite sides of all the zones of formation of tropical cyclones, and that the only ocean in which there is no region flanked by opposite currents is the south Atlantic, where these storms do not occur. What probably happens is that the meeting of these winds leads to the formation of eddies and ascending currents, and that these form an important stimulus in deciding the locality of formation of cyclones according to the convectional theory. The most probable explanation is to be found therefore in a combination of the two theories.

RELATION OF WEATHER CONDITIONS TO WIRELESS AUDIBILITY.

By M. P. BRUNIG.

[Nebraska Wesleyan University, University Place, Nebr., July 20, 1922.]

SYNOPSIS.

This article gives a brief résumé of work previously accomplished showing the relation between meteorological conditions and wireless audibility. Diagrams and explanation of a similar experiment carried on at Nebraska Wesleyan University are then given, and the curves obtained by means of the experiment show no relation between barometric pressure and audibility, no influence of conditions at sending station on audibility conditions at a distant receiving station, but do show that high static frequency, high static audibility, and a near-by thunderstorm area tend to reduce the audibility at the receiving station.

In the early investigations in the receiving of wireless messages it was noted that atmospheric disturbances greatly interfered with the receiving of signals at times and that the signals came in with varying intensities at other times with no apparent cause. This varying of signals, called "fading" or "swinging," was the subject of an investigation reported by J. H. Dellinger and L. E. Whittemore,[1] who found that the effect was more noticeable on land than on water, also more pronounced at night than in the daytime, although signals were stronger at night than in the daytime. In February, 1921, L. W. Austin [2] reported a series of observations on "The relation between atmospheric disturbances and wave length in radio reception," which gave the most complete information available at that time regarding atmospheric effects on signal receiving by wireless. Other important observations along this line were made by J. H. Dellinger and L. E. Whittemore,[1] Francis W. Reichelderfer,[3] E. W. Marchant,[4] and H. Mosler.[5] The static interference is worse at night than in daytime according to these and other investigators.

[1] *Jour. Wash. Acad. of Sci.*, vol. 11, No. 11, June, 1921.
[2] *Proc. Inst. Radio Eng.*, Feb., 1921.
[3] MO. WEATHER REV., Mar., 1921.
[4] *Electrician*, Feb., 1915.
[5] *Electrician*, Jan., 1914.

The present experiment, which was planned for the purpose of gathering more information concerning the relations between radio signal audibility and weather conditions, was begun in 1919, but it was not until the autumn of 1921 that final arrangements were made to take observations under controlled conditions. The circuit shown in Figure 1 was used for these observations.

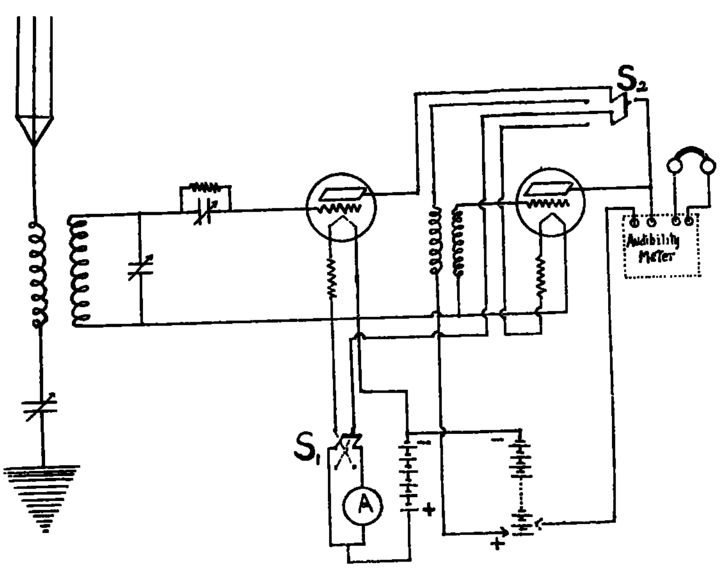

FIGURE 1.—Circuit used at Nebraska Wesleyan University for audibility measurements, 1921-22.

To insure constancy in the receiving conditions the following precautionary measures were taken: The tuning coil used was of the Navy type with contacts numbered and a scale and pointer adjusted so that exactly the same